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The revolution that was - or wasn't: A meta-analysis of the Keynesian revolution debate

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Arne Heise

**The Revolution that was — or wasn't:
A Meta-Analysis of the Keynesian
Revolution debate**

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Abstract

Following Lawrence Klein's designation of a 'Keynesian Revolution', the impact of John Maynard Keynes's *The General Theory of Employment, Interest and Money* has remained contested. While early interpretations viewed it as a refinement of neoclassical equilibrium theory, later accounts increasingly characterized it as revolutionary break. Drawing on a meta-synthesis of journal articles explicitly addressing the Keynesian Revolution, this paper shows that such claims are typically justified *intuitively* rather than on explicit philosophy-of-science grounds, even after Thomas S. Kuhn's *Structure of Scientific Revolutions*. Where analytical criteria are applied, conclusions hinge on whether Keynes's theory transforms the ontological and epistemological foundations of economics or merely modifies them. The findings suggest that most interpretations reflect intra-paradigmatic variation rather than a genuine scientific revolution suggesting 'a revolution that never was'.

JEL codes: A 11, B 20, B 31, E 12

Key words: Keynesian Revolution, Thomas S. Kuhn, Say's Theorem, Walras's Law

1. Introduction

One year after the death of John Maynard Keynes in 1946 and roughly a decade after the publication of his major work, *The General Theory of Employment, Interest and Money*, the American economist Lawrence Klein (1947) attributed an outstanding role to Keynes's work in the history of economic thought: Klein coined the term '*Keynesian Revolution*', which has since become an established element in the historical periodization of economics alongside the 'marginalist revolution' and the 'monetarist (counter-)revolution'¹.

This interpretation contrasted with influential early readings of the *General Theory*, such as those by John R. Hicks (1937), Alvin Hansen (1936), and Franco Modigliani (1944) among other. Their contributions — later forming the basis of the neoclassical synthesis— tended to interpret Keynes's 'new economics' as a development *within equilibrium theory*, suggesting *continuity* rather than *rupture* with prevailing economic thought.

Disagreement about the historical significance of theoretical developments is, of course, common. Even the shift from classical political economy in the tradition of Adam Smith, David Ricardo, and Karl Marx to neoclassical general equilibrium theory — the so-called marginalist revolution — is not unanimously regarded as a genuine theoretical break². Nevertheless, the divergent early interpretations of the *General Theory*, especially given that some initial sceptics later accepted its characterization as a 'revolution', raise the question of how this assessment was justified.

This question is particularly relevant because, prior to the publication of *The Structure of Scientific Revolutions* by Thomas S. Kuhn in 1962, no broadly influential and systematic theory of structural breaks in scientific knowledge existed (Kuhn 1962)³. The attribution of the label 'revolution' was, therefore, largely *intuitive*. It is, thus, necessary to examine both how this intuition was justified and whether, after Kuhn's work and the subsequent philosophy-of-science debates, the evaluation of the *General Theory* became more analytically grounded.

The present study first considers the concept of a scientific revolution and its use before and after Kuhn's contribution. It then surveys academic journal articles indexed in Google Scholar whose titles refer explicitly to the 'Keynesian Revolution'. The aim is to identify the criteria used to determine whether the *General Theory* should indeed be interpreted as a revolution and to assess whether these criteria — or the resulting evaluations — changed after 1962.

Beyond clarifying the reception of Keynes's work, this investigation also addresses the frequently voiced criticism that economists, despite their methodological ambitions (see e.g.

¹ The aforementioned book by Lawrence Klein is based on his doctoral dissertation of the same title, which he had already submitted at the Massachusetts Institute of Technology in 1944. As will be shown later, Klein was not the first to employ the term 'revolution' in reference to the work of John Maynard Keynes. However, he was the one who explicitly and affirmatively applied the designation '*Keynesian Revolution*' to *The General Theory of Employment, Interest and Money*, thereby firmly anchoring this characterization in the history of economic thought.

² See e.g. the contrasting views of Mark Blaug (1978: 322) and Mirowski (1984: 362f.).

³ Jean-Claude Jalladeau (1978) refers to the French philosopher Gaston Bachelard, who had already drawn attention to discontinuities in the process of scientific knowledge at an earlier stage. However, Bachelard did not develop a theory of scientific revolutions comparable to that later advanced by Thomas S. Kuhn. In any case, his work exerted relatively little influence on the philosophy of science outside France.

Colander 2005: 184f.), often shows limited engagement with philosophy-of-science debates (see, for example, Lawson 1994; Drakopoulos 2014). Ultimately, it seeks to determine whether Keynes's expectation that the General Theory would trigger a scientific revolution has been regarded as fulfilled within the economics profession.

2. Reflections on scientific revolutions before and after Thomas S. Kuhn

2.1 General considerations

Until well into the twentieth century, the prevailing view held that scientific knowledge develops in a *linear* and *incremental* manner. Stimulated either by internal scrutiny or by external developments — whether social, technological, or natural — existing bodies of knowledge are reassessed and, where necessary, revised or supplemented by new claims. This process leads to an accumulation of knowledge that causes the tree of knowledge to become ever more densely branched while relegating earlier bodies of knowledge and their analytical and theoretical foundations to the realm of the history of ideas and doctrines.

Such a conception of scientific development must essentially be described as *evolutionary*. In this perspective, 'revolutions' can at most refer to *shifts in perspective* perceived as radical, to *methodological innovations*, or to *particularly noteworthy expansions* of knowledge. However, these developments need not be *incommensurable* with, nor *unprecedented* in relation to, previous modes of inquiry.

A systematic philosophy-of-science foundation for an alternative view was provided only by the ground-breaking work of Thomas S. Kuhn, who replaced the notion of continuous — evolutionary — scientific development with a conception characterized by structural breaks. According to this perspective, scientific revolutions involve a *shift in the paradigm* generating knowledge, the *thought style* (*Denkstil*) described by Ludwik Fleck, the *research programme* of Imre Lakatos, or the *research tradition* identified by Larry Laudan⁴. Such a shift need not occur rapidly, despite the connotations of the term 'revolution'. Rather, it often takes the form of a prolonged struggle for interpretative authority extending over decades and across generations of researchers.

When successful, a scientific revolution entails a transition to an explanatory framework that is *incommensurable* with the previous paradigm and grounded in a new *pre-analytic vision* — or, in Kantian terms, a new *synthetic a priori judgment*⁵. The ontological dimension of the new paradigm is reflected in its epistemological articulation, namely in the core axioms that define its heuristic. The methodological dimension — the conventions governing which methods are considered appropriate for generating scientific knowledge — diverges from that of the

⁴ Philosophers of science continue to debate to this day the relationship between the various approaches of Thomas S. Kuhn, Imre Lakatos, Ludwik Fleck, Larry Laudan, and others. My concern here is not with the differences in detail and semantics among these approaches, but rather with their shared insight into a plural foundation of knowledge, characterized by struggles over interpretive authority, empirical and logical moments of crisis, and the countervailing forces of persistence that together shape the terrain of scientific contestation. I usually refer generally to Kuhn and use his notions because of their prominence, yet rely analytically more on Imre Lakatos's structure of scientific research programmes for reasons of clarity and consistency.

⁵ See Weber (1999: 117ff.) for the claim that scientific revolutions are grounded in ontological revisions.

previous paradigm primarily when the transformation also involves *disciplinary differentiation*. Such differentiation may occur, for example, when broader fields fragment into *specialized subdisciplines* within an ongoing process of *professionalization* and increasing division of labour⁶.

Clearly distinct from such *paradigm shifts* are the far more common *modifications* that occur *within* normal science and form part of the resilience structure of a paradigm. These changes typically involve extensions or adjustments — especially at the methodical level or within the protective belt of assumptions surrounding the core axioms. This process of intra-paradigmatic variation, which Fleck described as the result of a ‘thought style compulsion’, may produce developments colloquially described as ‘revolutionary’, but which, from a philosophy-of-science perspective, clearly are not (see Fleck 1979 and fn 8)⁷.

2.2 Reflections on Economics

It is well known that Thomas S. Kuhn himself was uncertain whether his concept of scientific revolutions — developed primarily with the natural sciences in mind — could be applied to the social sciences. The main reason for his scepticism was the difficulty of determining whether the social sciences could already be regarded as paradigm-oriented sciences or whether they remained in a comparatively ‘immature’, pre-paradigmatic stage of development.

Kuhn’s notion of a ‘mature’ science is moreover mono-paradigmatic: research activity is grounded in paradigms with clearly identifiable ontological, epistemological, and methodological dimensions, and the scientific community agrees upon a common paradigm—the domain of ‘normal science’. Under such conditions, however, scientific revolutions

⁶ In the German *Historischer Methodenstreit* (‘historical dispute over methods’) within the *Verein für Socialpolitik*— the principal gathering of the German economics profession — this process of disciplinary diversification becomes particularly evident. In this debate, representatives oriented toward the positivist methodology of the natural sciences confronted proponents of the *historical school*, which was especially influential in Germany. Volker Kruse (1990) traces how this controversy led, on the one hand, to the emergence of *Nationalökonomie* (economics) increasingly shaped by positivist methodology and, on the other hand, to the development of German (economic) sociology as an independent discipline. However, an influential branch of the historical school continued to persist within economics well into the twentieth century, just as there were also scholars within sociology who sought to anchor positivism within their discipline. See, among others, Loužek (2011: 459) and Trigilia (2002).

⁷ None of the concepts used here are uncontested. Both the notion of a *paradigm* and that of *incommensurability* are heavily debated in the philosophy of science, which makes it important to state one’s own interpretation explicitly. Surprisingly, this also applies to the concept of a *scientific revolution*. According to Ladislav Kvasz (1999), there are at least three different types of scientific revolutions: the ‘Russian’, the ‘French-British’, and the ‘American’ scientific revolution. The ‘Russian scientific revolution’ completely replaces one scientific regime—one idealization—with a new one. The ‘French-British scientific revolution’, by contrast, does not enthrone a new scientific regime, but merely alters its representation. Finally, the ‘American scientific revolution’ does not replace an existing scientific regime at all, but instead establishes a different scientific apparatus as an autonomous and independent regime in its own right. In terms of Lakatosian research programmes, the ‘Russian scientific revolution’ could be identified with a change in the pre-analytical vision at the *ontological level*, whereas the ‘French-British scientific revolution’ corresponds to axiomatic variations at the *epistemological level*, and the ‘American scientific revolution’ refers to disciplinary diversification through *methodological differentiation*. As helpful as these extensions of the concept of revolution may be in making sense of its diverse usage, they are ultimately problematic for a consistent philosophy-of-science analysis of the development of systems of knowledge production. For this reason, only the ‘Russian variant’ will be accepted here as a *genuine* scientific revolution.

become unlikely, since scientists have little incentive to develop alternative paradigms if doing so risks neglect or marginalization.

The conception proposed by Imre Lakatos, involving dominant ('progressive') research programmes, appears better suited both to the occasional occurrence of scientific revolutions and to the philosophy-of-science imperative of pluralism. Such programmes exert a homogenizing and disciplinary influence while still allowing competing research programmes to coexist. In this perspective, a scientific revolution does not necessarily entail the complete replacement of a formerly dominant paradigm; rather, it signifies a shift in dominance between competing paradigms as they are judged by the scientific community to be 'progressive' or 'degenerating'⁸.

At least economics, as a discipline within the social sciences, clearly conceives of itself as paradigmatic. With the increasing dominance of the United States as the hegemon of scientific research — often described as the 'Americanization' of economics (see e.g. Coats 1997, Hesse 2012) — a form of paradigmatic monism has emerged in which neoclassical economics has become the dominant framework. This standard economics, largely shaped by a small number of elite U.S. universities, disseminated through a limited set of highly ranked journals, and canonized in a few globally influential textbooks, functions as Kuhnian 'normal science' (see e.g. Fourcade 2006; Fourcade, Ollion and Algan 2015). Alongside this dominant paradigm, heterodox approaches persist largely in a marginalized position (see e.g. Heise 2016). A closer examination of the history of economic thought shows that the so-called 'marginalist revolution' at the turn of the twentieth century is not considered revolutionary because it generated insights fundamentally incompatible with classical political economy.

Rather, neoclassical economics introduced a different perspective—short-run, subjective price determination as an equilibrium mechanism instead of long-run, objective value determination as a gravitational centre — and adopted a methodology and heuristic inspired by physics as the leading *Leitwissenschaft*. In particular, the increasing mathematization of economics formalized the process of knowledge generation in unprecedented ways (see Philip Mirowski 1984).

⁸ However, some qualifications are necessary. Thomas Kuhn focused on *scientific revolutions* as a particular form of scientific progress and coined the term *paradigm* to emphasize the incommensurability of competing frameworks of knowledge production, although he was not always precise about what exactly *incommensurability* entails (see Worrall 2025: 249ff.). Imre Lakatos, by contrast, focused on *scientific progress* in terms of competing research programmes, distinguishing between those that become *degenerating* and those that remain *progressive*. Here, the emphasis lies less on the incommensurability of competing programmes than on the criteria by which degeneration and progressiveness are identified. However, if one adopts the dimensions of Lakatosian research programmes, it can be shown that the succession of *progressive* research programmes does not necessarily imply a scientific revolution in the Kuhnian sense. Only if different research programmes are grounded in different pre-analytic visions at the ontological level are they genuinely incommensurable, in which case their succession can unambiguously be described as a scientific revolution. If, by contrast, the differences concern only the epistemological or methodical dimensions, the competing programmes remain commensurable. In that case, the transition would amount merely to variations within what Kuhn would call a single paradigm, rather than to a genuine scientific revolution. For this reason, scientific revolutions can best be analysed by combining Kuhn's and Lakatos's insights. It is, therefore, crucial to distinguish carefully between paradigm shifts and changes in research programmes.

The term 'revolution' is, therefore, often used in an *intuitive* and *colloquial* sense, equated with what appears 'new' or 'unprecedented' without an explicit ontological justification. From the philosophy-of-science perspective outlined here, the marginalist revolution does not entail an ontological redefinition of the object of analysis — the capitalist system conceived as an exchange or market economy — relative to the classical understanding (as the prefix 'neo' suggests). At most, it can be interpreted as a variation or, in the terminology of Ludwik Fleck, an extension or modification of the prevailing thought style (see e.g. Blaug 1978: 322, Bowley 1973, Chap.4; Routh 1975: 24ff.).

3. 'Normal Science' in economics – paradigmatic foundations

Taking a 'scientific revolution' as a paradigm shift, a dominant paradigm must previously have existed and later been replaced by a new one that becomes the basis of normal science. In economics, as noted above, such dominance emerged only after the Second World War — largely in the United States — when the paradigm commonly referred to as neoclassical economics began to prevail. More precisely, however, it may be described as *general equilibrium economics*, tracing back to the works of William Stanley Jevons, Léon Walras, Carl Menger, and Alfred Marshall, and later formalized by economists such as Kenneth Arrow, Frank Hahn, and Gérard Debreu.

For the neoclassical paradigm, ontological, epistemological, and methodological dimensions must be identifiable that are sufficiently general to accommodate theoretical variations while remaining specific enough to distinguish the paradigm from competing, incommensurable frameworks. Determining the methodological dimension poses the least difficulty. Since the historical *Methodenstreit*, economics has largely adopted a form of (fallibilist) positivism — appearing in variants such as critical rationalism or realism — as the meta-method of a discipline that seeks to emulate the natural sciences by explaining phenomena through theory-based predictions that must withstand empirical testing.

If this methodological orientation characterizes the discipline rather than a specific paradigm, the distinguishing features of the neoclassical paradigm must lie in its *ontological* and *epistemological* dimensions: its heuristic core defined by inviolable axioms and its pre-analytical foundation.

The ontological dimension — although often neglected in economic theorizing — is crucial because it determines how economists conceptualize their object of inquiry. Ontology here refers to the existential nature of an entity expressed through its constitutive attributes that determine the empirically observable outcomes of the system under investigation. These attributes are not logically contained within the system itself but arise from what Immanuel Kant termed a *synthetic a priori judgment* or, in the terminology of Joseph Schumpeter, form a *pre-analytical vision*. From this perspective, ontology necessarily incorporates normative orientations toward the object of study, implying the existence of multiple possible pre-analytical visions. Because such visions cannot be derived directly from observation, they typically remain implicit and preconceptual, yet they shape the development and explanatory potential of theories and models (see e.g. Benedetti and Solari 1997).

Historically, economic thought has largely treated exchange — trade or barter conducted in markets and mediated by money — as the fundamental element of economic life. Economics is therefore commonly understood as the discipline analysing these exchange processes at the microeconomic level and their aggregate outcomes in macroeconomics and political economy.

Most orthodox textbooks rarely address these ontological foundations explicitly. Instead, their structure implicitly reflects the market or exchange paradigm: one part devoted to microeconomic analysis of markets and another to macroeconomic issues such as growth, distribution, inflation, and unemployment. Economics as practised by the mainstream and functioning as Kuhnian normal science can therefore be described as a market or exchange paradigm. Within this framework, exchange relations form the central object of analysis and the perspective is predominantly allocative — what Giulio Palermo (2007: 540) characterizes as a “what-you-see-is-what-you-get ontology,” one that largely neglects deeper structural dimensions of the economic system.

If this pre-analytical vision of a market economy is combined with assumptions of rational utility-maximizing behaviour and the availability of the information required for rational decision-making — including forward-looking decisions based on rational expectations — the resulting economic system can plausibly be described as a *closed system* (see Tony Lawson 2009: 99). In such a system all relevant elements and relationships are known, outcomes are deterministic, and complexity remains limited.

At the epistemological level, this ontology corresponds to a set of core axioms. Paul Davidson (2009: 26ff.) identifies three fundamental axioms of the real exchange paradigm⁹.

1. the axiom of (gross) substitutability
2. the axiom of ergodicity
3. the axiom of monetary neutrality

The first underpins the allocative orientation of the paradigm, the second secures its deterministic structure, and the third establishes the dichotomy between the real and the monetary sphere. The real economy operates according to utility-maximizing behaviour — what Deirdre McCloskey (2016) describes as ‘Max U’ — while the monetary sphere concerns nominal magnitudes without affecting real economic laws.

To operationalize ‘Max U’, a further axiom is required:

4. the axiom of rationality.

Within this framework, Say's Law and its neoclassical counterpart, Walras's Law, function as the accounting principles of exchange economies and give rise to the familiar corollaries of *self-regulation* and *general equilibrium*.

Additional assumptions — such as imperfect competition, asymmetric information, adaptive expectations, institutional rigidities, transaction costs, or behavioural norms beyond strict utility maximization — as well as greater emphasis on dynamics and disequilibrium generate

⁹ In doing so, he draws on the foundational works of the pioneers of general equilibrium theory: Arrow and Hahn (1971), as well as Hahn (1958), Hahn (1962) and Hahn (1977).

paradigmatic variants such as neo-institutional, New Keynesian, evolutionary, or behavioural approaches. These variants address specific markets and phenomena such as unemployment, rationing, business cycles, or innovation.

In summary, standard neoclassical economics rests on a *real exchange ontology* that conceives the economic system as closed, assumes rational actors, and distinguishes between real and nominal exchange relations. Such a system is viewed as *self-stabilizing* and tending toward *general equilibrium*, implying the *validity of Say's Theorem and Walras's Law*. At the epistemological level, variations in auxiliary assumptions and methods generate a wide spectrum of theories and models which — despite apparent differences in policy implications or methodological orientation — remain mutually commensurable¹⁰.

This also implies that any genuine alternative to the standard exchange or market paradigm must rest on a different pre-analytical vision – i. e. a different ontological foundation – whose epistemological configuration abolishes the validity of Say's Theorem and Walras's Law.

4. Keynes's genuine reflections on the significance of the *General Theory*

It is widely known that John Maynard Keynes himself claimed that the publication of the *General Theory* was intended to trigger a scientific revolution (Keynes 1935a). In his 1935 essay *A self-adjusting economic system* he distinguishes between two 'schools' separated by an abyss which "is deeper, I believe, than most of those on either side of it realize" (Keynes 1935b: 35). He characterizes these two schools as follows:

"On the one side were those who believed that the existing economic system is in the long run self-adjusting, though with creaks and groans and jerks, and interrupted by time-lags, outside interference and mistake. ... Those on the other side of the gulf, however, rejected the idea that the existing economic system is, in any significant sense, self-adjusting. They believed that the failure of effective demand to reach the full potentialities of supply, in spite of human psychological demand being immensely far from satisfied for the vast majority of individuals, is due to much more fundamental causes" (Keynes 1935b: 36).

Keynes clearly positions himself within the second school and, therefore, characterizes himself as a 'heretic' who intends "to demolish the forces of nineteenth-century orthodoxy - and I include Marxism in orthodoxy equally with laissez-faire, these two being the nineteenth-century twins of Say and Ricardo - they must attack them in their citadel" (Keynes 1935b: 36).

If these statements were to be interpreted as expressing a revolutionary claim, it must nevertheless be noted that Keynes does not provide a philosophical analysis or justification of this claim¹¹. Rather, he points to school-specific — or, as we would say today, paradigm-

¹⁰ In a recent paper, Olivier Blanchard (2025) not only confirmed his assessment made 16 years earlier that the state of the economic discipline is good but that it converged around a mainstream 'Dynamic Stochastic General Equilibrium Model' which he takes as the 'normal science' of a mature economic science.

¹¹ At this point, it is necessary once again to recall the particular condition of economics during the first decades of the twentieth century: on the one hand, no monistic paradigm had yet fully crystallized; on the other hand, within the discipline there was a growing tendency—especially in the aftermath of the Methodenstreit, and reinforced by Alfred Marshall's and the University of Cambridge's strong position for the international community of economic scholars (see e.g. Marcuzzo et al. 2008) — to favour a marketoriented

immanent — characteristics that manifest profound, indeed incommensurable differences: *self-regulation versus the rejection of self-regulation* as a fundamental postulate. He, thus, portrays economics as engaged in a long struggle for interpretative dominance, in which the ‘orthodoxy’ had so far succeeded in marginalizing the heretics as scientific cranks — primarily because the latter had relied on the erroneous belief “that common observation is enough to show that facts do not conform to the orthodox reasoning” (Keynes 1935b: 36).

Keynes, therefore, already understood what Thomas S. Kuhn would confirm decades later: empirical falsification is a *necessary* but not a *sufficient* condition for abandoning established knowledge. What is additionally required is the availability of a *theoretical alternative* and thus the transformation of existing bodies of knowledge:

“I believe that I am on my way to do so. There is, I am convinced, a fatal flaw in that part of the orthodox reasoning that deals with the theory of what determines the level of effective demand and the volume of aggregate employment; the flaw being largely due to the failure of the classical doctrine to develop a satisfactory and realistic theory of the rate of interest” (Keynes 1935b: 36).

In the *General Theory*, as well as in letters to close fellows (e.g., Keynes 1934a: 422), Keynes makes clear that he regarded his theory as revolutionary because it rejected “(t)he fundamental assumption of the classical theory, ‘supply creates its own demand’...” — *that is, Say’s Law*. Central to this rejection, at the epistemological level, is the ‘*principle of effective demand*’ outlined in Chapter 3 of the *General Theory* (Heise 2025), being “...the substance of the General Theory,...” (Keynes 1936: 25).

However, this key chapter only assumed its final form — essentially as a summarizing anticipation of the analysis that follows — around mid-1934. As late as the end of 1933, the introductory Book I of the *General Theory* still contained preliminary reflections on the *ontological foundations* of Keynes’s object of inquiry rather than merely this *epistemological prolepsis*. Apparently sensing intuitively that a new school of thought fundamentally diverging from the dominant paradigm must be based on an alternative pre-analytical vision, Keynes distinguished his conception of the world in which we live — an ‘*entrepreneur*’ or ‘*monetary production economy*’ — from the standard interpretation of the economy as a ‘*barter*’, ‘*real-wage*’, or ‘*co-operative economy*’ (see Keynes 1933a; Keynes 1933b).

Drafts outlining the characteristics of these different social imaginaries do exist and would undoubtedly have greatly facilitated later generations’ understanding of Keynes’s revolutionary intentions. Unfortunately, for reasons that remain unknown, they were not included in the *General Theory*. They were discovered only in the mid-1970s and published toward the end of that decade — by which time the monetarist counter-revolution had already undermined the impetus of the Keynesian revolution.

Keynes was fully aware that his struggle against the orthodox citadel would not achieve immediate success, and that many of his academic colleagues “will fluctuate, I expect, between a belief that I am quite wrong and a belief that I am saying nothing new” (Keynes

equilibrium perspective. In positioning himself specifically against this tendency, John Maynard Keynes apparently sought to influence the course of the history of economic thought at a particularly decisive moment.

1936: XXI). Nevertheless, he remained convinced of the power of — his — ideas and hoped “to be breaking away from this orthodoxy”, emphasizing that “it is my fellow economists, ..., whom I must first convince” (Keynes 1936: XXXI and XXI).

5. The Keynesian Revolution and the development of the adaptation of the *General Theory*

At the latest following the publication of *The Economic Consequences of the Peace*, John Maynard Keynes had become an internationally renowned economist whose subsequent work was expected to attract global attention. Nevertheless, it was by no means clear — and this was already anticipated by Keynes himself — that the *General Theory* would, only a decade after its publication, be honoured with the acclaim of having revolutionized economics.

The following section therefore examines how the views expressed in scientific journals

— that the publication of the *General Theory* in 1936 triggered a ‘Keynesian Revolution’

— have been justified¹². At the outset it must be noted that this concept may refer equally to a change in the *scientific paradigm* and to a change in the *economic policy paradigm*. Whereas the notion of a scientific paradigm shift, as discussed above, is based on Thomas S. Kuhn’s theory of scientific revolutions, the concept of an economic policy paradigm shift refers to Peter A. Hall’s conception of policy paradigms as “a framework of ideas and standards that specifies not only the goals of policy and the kind of instruments that can be used to attain them, but also the very nature of the problems they are meant to be addressing” (Hall 1993: 279). In this context, a policy paradigm shift refers to a third-order change, which implies not merely a change in the settings of existing instruments (first order) or the replacement of instruments (second order), but a complete transformation of the underlying goal–instrument framework (third order). Although the two notions of paradigms are often used synonymously, or policy paradigm shifts are derived from preceding scientific paradigm shifts, this close interaction between the two forms of paradigm change is not easily justified. Economic policy paradigm shifts may also be triggered by intra-paradigmatic variations (see

¹² In the sense of a meta-synthesis (see e.g. Jensen and Allen 1996; Walsh and Downe 2005), a comprehensive survey is undertaken here. Such an objective can seriously be fulfilled only if the survey is limited to scientific journal articles, excluding book chapters and especially op-eds, and only to those contributions that explicitly indicate — through the mention of the ‘*Keynesian Revolution*’ in the title — that this is indeed the subject of the publication. Naturally, there exist numerous publications that in one way or another address the Keynesian Revolution or at least refer to it. However, if this reference is not reflected in the title, it is assumed here that the Keynesian Revolution was not the principal object of investigation in the respective article. Moreover, purely book reviews could not be taken into account. The comprehensive survey is conducted using the internet-based search engine *Google Scholar*. This search engine is employed because, unlike curated databases or repositories such as Web of Science or JSTOR, it offers a greater likelihood of complete coverage, as it is not subject to the indexing restrictions typical of curated databases. Nevertheless, an additional cross-check with the entries listed in Web of Science was carried out. Tomo Suzuki (2003) identifies 52 works (up to the reference year 1998) that refer to the ‘Keynesian Revolution’ in their titles— including books, journal articles, and book chapters. Up to the year 1998, we have identified 35 journal articles, which allows us to be reasonably confident that our claim of a comprehensive survey (of articles) has largely been fulfilled. The articles considered are compiled in Tables A.1 and A.2 in the appendix.

Heise 2025: 2f.). While the present analysis focuses on the Keynesian Revolution as a scientific structural break, attention will also be paid to whether and where references are made to both types of paradigm change¹³.

5.1 Assessments Prior to the Publication of *The Structure of Scientific Revolutions*

When Keynes's *General Theory* appeared in 1936 after several years of gestation, it was immediately taken up with great interest by the scholarly community — though initially almost exclusively with a critical undertone¹⁴. Arthur Cecil Pigou (1936) criticized the book so harshly that after Keynes's death he felt compelled to produce a more conciliatory version of his critique (Pigou 1950). Benjamin Haggott Beckhart (1936: 603) regarded the *General Theory* as

“an interesting exhibit in the museum of depression curiosities”,

while Gustav Cassel (1937: 437f.) argued that

“Keynes' exposition is defective, not only in observation of facts, but also in theoretical reasoning and in practical conclusions”.

Joseph Alois Schumpeter (1936: 791), by contrast, attempted to give his criticism a more conciliatory tone:

“The unfavorable reviews in a sense but testify to the reality of that success, and I for one, being about to write another of those unfavorable reviews, heartily rejoice in this implication and wish it to be understood that what I am going to say is, in its own unconventional way, a tribute to one of the most brilliant men who ever bent their energies to economic problems”.

Finally, John Hicks (1936; 1937) and Alvin Hansen (1936: 686) approached the *General Theory* by interpreting it as an extension of the neoclassical mainstream — an interpretation that later became the standard reading in the form of the IS–LM model:

“We are living in a time when economics stands in danger of a sterile orthodoxy. The book under review (the *General Theory*, A.H.) is not a landmark in the sense that it lays a foundation for a ‘new economics’. It warns once again, in a provocative manner, of the danger of reasoning based on assumptions which no longer fit the facts of economic life. Out of discussion and research will come bit by bit an improved theoretical apparatus (Keynes's interest theory contains promising suggestions) and a more accurate appreciation of social psychology (the brilliant chapter on long-term expectation) and of the precise character of the economic environment in which humans act as individuals and in groups. The book is more a symptom of economic trends than a foundation stone upon which a science can be built”.

These early critical assessments were written by economists who regarded themselves as representatives of the ‘classical economists’ from whom Keynes attempted so vehemently to distance himself in the *General Theory*. It is, therefore, hardly surprising that their initial reactions were formulated in a defensive mode. However, even economists from Keynes's

¹³ On the other hand, all those studies that deal exclusively with the economic policy paradigm shift will not be examined in detail here: Winch (1966), Gwartney and Stroup (1980), Tomlinson (1981, 1982, 1984), Smith (1982), Booth (1983, 1984, 1985), Peden (1983), Rollings (1985, 1988), Cornish (1993), Baumol (2014), and Metinsoy (2021).

¹⁴ As James Tobin (1988: 36) notes, the *General Theory* „was getting a frosty reception in the profession”.

immediate intellectual circle — such as Abba Lerner and Roy Harrod — whose assessments cannot be interpreted as reactions of injured pride, believed that

“Mr Keynes has not affected a revolution in fundamental economic theory but a re-adjustment and a shift of emphasis” (Harrod 1937: 85),

and that

“Keynes is so keen on making clear the difference between the classical and his own scheme that he *perhaps over-emphasises it*, willingly taking this risk in order to be certain of avoiding the other error, which he considers more dangerous, of permitting a reader to overlook the revolutionary nature of the change” (Lerner 1936: 436, my italics).

A first direct engagement with the claim of revolutionary character appears in a review of the *General Theory* by the Chicago economist Frank Knight. He somewhat wryly confirms Keynes’ prediction that economists firmly anchored in orthodoxy would either fail to recognize anything new in the *General Theory* or find it entirely mistaken:

“The prediction has been largely correct in my own case, though I should say that my difficulty (and no little annoyance) has been that of choosing between interpretations, one apparently non-sensical and the other more or less commonplace” (Knight 1937: 122).

Knight, therefore, rejects the revolutionary character of the *General Theory*, accepting it at most as a contribution to business cycle theory:

“Now I for one simply cannot take this new and revolutionary equilibrium theory seriously, and doubt whether Mr. Keynes himself really does so. ..., it is my own conviction that we must simply ‘forget’ the revolution in economic theory and read the book as a contribution to the theory of business oscillation” (Knight 1937: 121).

Lawrence Klein (1944; 1947) is widely regarded as the originator of the affirmative use of the term ‘Keynesian Revolution’¹⁵, marking the phase in which the *General Theory* began to be appreciated for its impact. Klein describes it as

“a complete turn-about in economic theory *and policy*” (Klein 1944: 62, italics in original)

and explains:

“The Revolution, then, is simply the replacement of the accepted, classical savings-investment theory of interest by the Keynesian savings-investment theory of effective demand or employment” (Klein 1944: iii).

By substituting the interest rate in the savings-investment functions with income level — later complemented in the ISLM model — Klein identifies a significant *shift in perspective*: from microeconomic, allocative concerns to macroeconomic, income- and employment-determining ones. Yet he acknowledges that this alone does not constitute a scientific revolution. He sees the decisive step in Keynes’ analysis as the combination of income-determined savings-investment equilibrium with the multiplier analysis, and the rejection of Say’s Law:

¹⁵ And with this prominent position, it is justified that Klein (1944; 1947) should be regarded here as the only monograph.

"If we can find a theory which describes a non-self-adjusting capitalistic, friction-less, perfectly competitive system, have we not found a revolutionary theory? ... The revolution was solely the development of a theory of effective demand; i.e. a theory of the determination of the level of output as a whole. ... We know that Keynes' procedure was first to do away with the savings-investment theory of interest and replace it with a savings-investment theory of output. When this step had been done, the Revolution was *a fait accompli*, ... He gave up Say's law" (Klein 1944: 56, 72, 93; italics in original).

We know today that Klein's epistemological additions alone are insufficient to refute Say's Law consistently¹⁶: beyond the *hoarding of money*, which may at best be considered temporarily rational behaviour, *supply in a barter economy cannot be constrained by demand*. Although Klein, through his focus on effective demand and the multiplier principle, combined with his simultaneous rejection of an alternative interest-rate theory¹⁷, deprived himself of the possibility of taking Keynes' arguments as the basis for a principled refutation of Say's Theorem, he nevertheless identifies a *necessary* — and also *sufficient* — condition for designating a theory as a revolutionary innovation relative to the established orthodoxy¹⁸.

Judging by Google Scholar entries, Klein's assessment — that the *General Theory* was revolutionary in both theoretical and policy terms — initially found little resonance. Up to 1962, the year of Kuhn's seminal work on scientific revolutions, only three journal articles explicitly discuss the 'Keynesian Revolution'¹⁹:

¹⁶ This is not the place to trace the debates surrounding the interpretation of Say's Theorem or the distinctions between 'Say's Theorem,' 'Say's Law of Markets,' and Say's identity. It suffices to cite, following Kates (2019: 127), the propositions that constitute Say's Theorem: "1. Recessions do occur and when they do, the effect on the labor market is often prolonged and devastating. 2. Recessions are not, however, caused by oversaving and demand deficiency. 3. Recessions cannot be brought to an end through increases in aggregate demand."

¹⁷ "Keynes did not present at this time a complete outline of his theory, but he did say enough to indicate the core of the analysis. ... He claimed that the inability of the classical system to determine the level of effective demand and of employment was due to an unsatisfactory interest theory. Are we to take this to mean that the Keynesian Revolution lies in the interest theory – liquidity preference? We can admit that the theory of liquidity preference is an ingenious solution to the problem of the determination of the rate of interest, which is *essentially* different from the classical, neo-classical or modern Swedish theories, but we need not regard the liquidity preference theory as an essential element of the Keynesian system (Klein 1944: 56; italics in original). However, in some articles elaborating the *General Theory*, Keynes (1937a; 1937b) is quite explicit about the crucial relevance of his liquidity preference theory (see also Kregel 1985, Heise 2026: Kap. 4 and 5).

¹⁸ Ludwig von Mises takes a similar view: „Lord Keynes's main contribution did not lie in the development of new ideas but "in escaping from the old ones," as he himself declared at the end of the Preface to his 'General Theory'. The Keynesians tell us that his immortal achievement consists in the entire refutation of what has come to be known as Say's Law of Markets. The rejection of this law, they declare, is the gist of all Keynes's teachings; all other propositions of his doctrine follow *with logical necessity from this fundamental insight and must collapse if the futility of his attack on Say's Law can be demonstrated*" (Mises 1952: 64, my italics).

obscure the fact that Keynes did not refute Say's Law. He rejected it emotionally, but he did not advance a single tenable argument to invalidate its rationale" (Mises 1952: 70). Needless to say, Ludwig von Mises regarded precisely this—the rejection of Say's Theorem — as having failed to be achieved consistently, and therefore considered Keynes' seemingly revolutionary views to have ultimately failed: "The exuberant epithets which these admirers bestowed upon his work cannot

¹⁹ After John Maynard Keynes's death in 1946, obituaries naturally appeared in all major economic journals, acknowledging the significance of his work (see, e.g., Roy Harrod 1946, Joseph Schumpeter 1946, Joan Robinson 1947), and occasionally employing the term "Keynesian Revolution" in this context (see Gottfried Haberler 1946; Alvin Hansen 1946). Yet none of these obituaries referred in their titles to the Keynesian Revolution as the culmination of his work, since they did not regard it as constituting a revolutionary contribution. Interestingly, the Marxist economist Paul M. Sweezy (1946) refers to Keynes' rejection of Say's

G.W.G. Browne (1946: 242, 243, 245) insists on the revolutionary character of Keynes' approach stemming primarily

"from his ,macro-economic' approach, in contrast to the classical micro-economic-theory...",

"the revolutionary conclusion that there is no direct and automatic tendency toward the establishment of full employment"

and the "consequences for policy".

Analytically, this is less advanced than Klein, whose dissertation, however, Browne might not have known.

J.S. Gambs (1947) rejects the revolutionary claim of the *General Theory*. While acknowledging that Keynes' work contributed business cycle innovations akin to contemporaneous studies by Chamberlin and Robinson, he does not engage with its epistemological structures — e.g., the validity of Say's Law — but criticizes the methodological emulation of physics and the focus on 'competitive equilibrium' as the guiding model:

"As I have said, the ideal of 'competitive equilibrium' is still the norm in the work of Keynes and the others. They realize, however, that there has been considerable backsliding from the norm. ... And so we find that neither the 'Keynesian revolution' nor the other novelties of standard economic theory in recent years, are very impressive from the institutionalist viewpoint. Much has been done that is good and interesting. Keynes's high intelligence, wit, and shrewd observation have enriched the literature of economics. ... All have dealt in certain limited ways so well with such realistic problems as monopoly and the business cycle that institutionalists deplore their not having taken the next step: complete repudiation of methods no longer useful. In a recent article, John M. Clark suggests that perhaps the 'Keynesian revolution' is only the prelude to a truly grandiose revolution in economic theory" (Gambs 1947: 401f.).

For Gambs, it is *methodological commitment* to a selectively adaptive, evolutionary dynamism — not epistemological differentiation — that defines a true revolution (cf. Veblen 1900) which, he believes, the *General Theory* has not achieved.

Dudley Dillard (1948) emphasizes that the term 'Keynesian Revolution' is often used interchangeably with 'New Economics,' referencing *general novelty* rather than *analytical depth*:

"...it will supplement rather than replace the neoclassical economics of Alfred Marshall."

Keynes' ideas, Dillard notes, build on long-established approaches to deficient demand from Thomas Robert Malthus and C.H. Douglas and extend through Karl Marx to Silvio Gesell. Dillard critiques the narrow use of the term 'Keynesian Revolution,' which focuses on the doctrinal history of economics rather than broader societal change:

Theorem — which, as we have seen, according to Klein would be a sufficient condition for a revolutionary claim — yet nevertheless downplays both Keynes' intention and achievement: "In fact, one would be perfectly justified in saying that Keynes is both the most important and the most illustrious product of the neoclassical school. This points, I think, is the true nature of Keynes' achievement. His mission was to reform neo-classical economics, to bring it back into contact with the real world from which it had wandered farther and farther since the break with the classical tradition in the nineteenth century" (Sweezy 1946: 400).

"In protesting against the narrow view of the Keynesian Revolution, I am, of course, protesting against the narrow view of economics ... The Keynesian Revolution is more than the invention of an abstract, logical set of relations. ... The Keynesian Revolution is a further step in the critical phase of a process of changing social and economic relations among individuals and groups. ... The interpretation of the Keynesian Revolution in the narrower sense - ... - tends to overstate the importance of Keynes to economics (in the narrower sense) and to understate his significance for those outside the field of economics" (Dillard 1948: 177).

In summary, the interpretive framework of the 'Keynesian Revolution' had not yet consolidated by the eve of Kuhn's analysis of scientific revolutions²⁰. While appreciation grew for Keynes' *shift from micro- to macroeconomics* — recognized as 'New Economics' not only for theoretical-epistemological innovations but also as a foundation for pragmatic post-Depression economic policy — this does not preclude understanding Keynesian economics as an *extension* of neoclassical standard economics or *ignoring* the central rejection of *Say's Law*, which remains the necessary and sufficient condition for a genuinely alternative analytical framework.

5.2 Assessments after the publication of *The Structure of Scientific Revolutions*

The 1960s witnessed two ground-breaking publications that are of particular relevance to our investigation. In 1960, Piero Sraffa published *Production of Commodities by Means of Commodities*, which he subtitled *Prelude to a Critique of Economic Theory* — by which he meant a critique of the neoclassical marginal analysis of value and distribution. Sraffa's work may be regarded as the most fundamental attack on the *internal coherence* of neoclassical equilibrium analysis and its postulate of self-regulation. He rejects the unequivocally negative relationship between factor prices and factor quantities — and, thus, a fundamental adjustment mechanism — as logically inconsistent. In doing so, he shakes the foundations of the dominant economic doctrine, although without constructively offering an alternative approach.

Yet such an alternative — a new paradigm — is indispensable if the demonstration of the logical inconsistency of a paradigm is to lead to a *paradigm shift* — a *scientific revolution* — and, thus, to a fundamental *transformation* of the existing body of knowledge. This, at any rate, was the insight provided by the second ground-breaking work of the 1960s: *The Structure of Scientific Revolutions* by Thomas S. Kuhn (1962). Kuhn thereby offered the first *analytical* foundation for something that had previously been asserted only in a *colloquial* and *intuitive* sense. He demonstrated that *logical inconsistencies* and *empirical anomalies* may push dominant intellectual frameworks into *crisis* and — if new approaches present themselves — may over time lead to the replacement of the old framework by a new one.

Sraffa's critique seemed to have plunged the prevailing neoclassical doctrine into such a crisis. At the same time, Keynes' *General Theory*, developed in response to the Great Depression — which may be understood as an 'empirical anomaly' for a system that claims to be self-

²⁰ This result stands in clear contrast to the assessment of Aslanbeigui and Oakes (2002: 10; italics in original): "In the decade following the publication of *The General Theory*, a consensus was reached that a revolution in economics had taken place". Unfortunately, the authors do not substantiate this claim, but rather take it as a basic assumption for their analysis of the emerging dominance of 'Keynesian economics'.

regulating²¹ — could now appear to provide precisely the new theoretical framework capable of substantiating the previously hesitant claim of a ‘Keynesian Revolution’. However, Kuhn and other philosophers of science such as Imre Lakatos and, above all, Ludwik Fleck emphasized the *resilience* of dominant paradigms. In Fleck’s terms, a ‘*thought style compulsion*’ enables the necessary stability and durability of knowledge systems: numerous defensive and repair strategies are undertaken in order to *avert* a paradigm shift.

Early ambiguities without philosophy-of-science foundation

In this sense, the retrospective perspective offered by Joan Robinson (1974) should be understood. Although she does not explicitly refer to the emerging philosophy-of-science literature, she nonetheless illustrates how the *assimilation* of Keynesian theory into the old orthodoxy was achieved through processes of *sublimation* and *vulgarization*:

“After the war, Keynes’s theory was accepted as a new orthodoxy without the old one being rethought. In modern textbooks, the pendulum still swings, tending toward its equilibrium point. Market forces allocate given factors of production between alternative uses, investment is a sacrifice of present consumption, and the rate of interest measures society’s discount of the future. All the old slogans are repeated unchanged” (Robinson 1974: 8).

Certain ambiguities and unnecessary borrowings from orthodox theory — such as the assumption of perfect competition — facilitated this assimilation, particularly since Keynes himself was allegedly not fully aware of the revolutionary implications of his ideas:

“... there were moments when we had some trouble in getting Maynard to see what the point of his revolution really was” (Robinson 1974: 7).

Robinson’s own assessment of the revolutionary core of the *General Theory*, however, leaves open whether she was able to identify those innovations that could consistently substantiate the epistemological claim that

“under laissez faire the private-enterprise system does not tend toward equilibrium in any way at all” (Robinson 1974: 10).

She characterizes the theoretical shift as follows:

“On the plane of theory, the revolution lay in the change from the *conception of equilibrium* to the *conception of history*; from the *principles of rational choice* to the *problems of decisions based on guess-work and conventions*. ... The other half of the Keynesian revolution was to recognize that, in an industrial economy, the level of prices is governed primarily by the level of money-wage rates” (Robinson 1974: 9, my italics).

Robinson (1974: 11) being probably aware that these somewhat cryptic references — to the importance of *uncertainty* and *nominal magnitudes* for Keynes’s *monetary production economy*²² — hardly met the epistemological requirements for an incommensurable paradigmatic alternative, therefore, notes:

²¹ Paul Sweezy (1953: 278) notes: “... the opportunity to which Keynes responded was essentially a crisis in traditional economics, a crisis both accentuated and laid bare by the Great Depression”.

²² As is well known, John Maynard Keynes (1932) referred to his approach in this way in preparatory drafts of the *General Theory* (Keynes 1933a; Keynes 1933b).

“The Keynesian revolution still remains to be made both in teaching economic theory and in forming economic policy”.

Before Robinson, it had already been Lawrence Klein (1964) who once again returned to his narrative of the Keynesian Revolution. In this work, however, he does not substantiate his claim that Keynes’s theory refutes Say’s theorem — which, of course, is so crucial for the revolutionary claim, yet has gained little acceptance by the community of economists — but merely advocates an epistemological specification of the (his) ‘Keynesian system’ in order to derive improved econometric models from it. As the apparent ‘father’ of the narrative, Klein does not seem to feel challenged to defend his assertion that this constituted a scientific revolution, for instance in light of the new insights into the philosophy of science provided by Thomas S. Kuhn. Instead, he transforms the *Keynesian Revolution* into the (macroeconomically) dominant *Keynesian model*, which he sought to extend and consolidate through elaboration in the sense of the normal-scientific ‘puzzle-solving’ process. Michel De Vroey and Pierre Malgrange (2012), nevertheless, point out that the core of the Keynesian Revolution — the demonstration that *involuntary* unemployment may represent an *equilibrium situation* and, thus, the rejection of Say’s theorem — could *not* be implemented consistently in Klein’s macroeconometric modelling.

The work of Robert Clower (1965) is remarkable for two reasons. First, it may be regarded as the starting point of a new interpretation of Keynes, which became known as the ‘new macroeconomics’, ‘disequilibrium theory’, or ‘rationing Keynesianism’. From a history-of-economic-thought perspective, Romain Plassard (2018) traces the genesis of this interpretation of the Keynesian Revolution. More importantly, however, Clower’s work belongs to the few contributions that attempt to uncover the *analytical foundations* of the revolutionary innovations of Keynesian theory by arguing that

“...either Walras’ law is incompatible with Keynesian economics, or Keynes has nothing fundamentally new to add to orthodox economic theory” (Clower 1965: 278, italics in original).

Walras’s Law — the modern version of Say’s Law (cf. Clower 1965: 275) — states that disequilibria in one market (for example, an excess supply in the labour market, i.e. unemployment) must correspond to off-setting disequilibria in other markets (for example, excess demand in goods markets), thereby generating pressures toward adjustment from two sides — i.e. on at least two markets. If the self-regulating mechanism that constitutes the central element of orthodoxy is to be rejected *as a systemic feature* — and this is how Clower interprets the revolutionary character of Keynesian theory — then the universal validity of Walras’s Law must be denied. Within the framework of his ‘dual decision hypothesis’, Clower, therefore, attempts to show that Walras’s Law holds only in equilibrium but not in disequilibrium — namely when transactions occur at disequilibrium prices (‘false-price trading’) and market rationing arises:

“...the dual-decision hypothesis effectively implies that Walras’ law although valid as usual with reference to notional market excess demands, is in general irrelevant to any but full employment situations” (Clower 1962: 292).

Irrespective of whether Clower's demonstration of the untenability of Walras's Law is logically consistent — and this has indeed strongly been contested²³ — he nevertheless made it clear that the revolutionary character of Keynesian theory cannot be grounded merely in changes of perspective or methodical innovations. Rather, it requires the establishment of an incompatible — or, in Kuhn's terminology, incommensurable — construct (a paradigm). *Even without explicit reference to Kuhn's philosophy of science, Clower thus reinforces the claim that the Keynesian Revolution must rest on an analytical foundation, a claim that appears largely to have been lost in Klein's work.*

Against this background, the discussions of the Keynesian Revolution that followed in the 1970s are somewhat surprising. Hardly any of these contributions refer to the debate on Kuhn's insights into scientific revolutions, which had by then already lasted more than a decade, nor do they pursue the analytical path opened by Clower in attempting to identify the essence of the Keynesian Revolution.

Harry G. Johnson (1971), for instance, defines it in entirely unspecific terms:

“(i)ts essence is unexpected speed of change” (Johnson 1971: 1),

and consequently identifies numerous revolutions in the history of economic thought— from the ‘Smithian’ and ‘Ricardian’ to the ‘Marginalist Revolution’ — as well as

“at least four discernible ‘revolutions’ that occurred in the late 1920's and in the 1930's” (Johnson 1971: 1f.).

The shift in theory and policy during the 1930s and 1940s is, thus, naturally described as the ‘Keynesian Revolution’ and elevated to the status of a new orthodoxy against which, in the 1960s — under the impact of apparent empirical anomalies (notably the phenomenon of ‘stagflation’) — rebellion emerged on the basis of the same narrative:

“on the one hand, the existence of an important social and economic problem with which the prevailing orthodoxy was unable to cope; on the other hand, a variety of characteristics that appealed to the younger generation of that period - notably the claim of the new theory to superior social relevance and intellectual distinction, its incorporation in a novel and confusing fashion of the valid elements of traditional theory, the opportunity it offered to bypass the system of academic seniority by challenging senior colleagues with a new and self-announcedly superior scientific approach, the presentation of a new methodology that made

²³ The controversial point lies, on the one hand, in whether the rejection of Walras's Law thus established is in fact plausible. This may be doubted, for in the distinction — so central to his dual decision theory — between *actual* and *notional* magnitudes, he considers only the labour market (where the rationed supply of labour receives less income than in the unrationed case), but not the goods market (where rationing in the labour market must necessarily be reflected in a corresponding rationing of goods or production). Palley (1998), Rhodes (1984), and Patinkin (1987) demonstrate that Walras's Law always holds in an exchange economy — irrespective of the level of income actually realised — whereas Harrison (1981) appears to dispute this. Moreover, it remains questionable whether, even if Walras's Law does not hold under disequilibrium conditions, an adjustment process would nevertheless have to occur — though in that case it could only be realised in the labour market through wage reductions — and whether the conclusion that the invalidity of Walras's Law implies a rejection of the self-regulation postulate would, therefore, be tenable. Romain Plassard (2017: 284) remarks somewhat laconically: “He was an ambitious economist, asked important questions to understand the functioning of market economies, and always provided promising intuitions to answer. But he rarely succeeded in formalizing the models that fully supported his views”.

generalequilibrium theory both manageable and socially relevant, and the advancement of a new empirical relationship challenging for econometricians to estimate” (Johnson 1971: 5f.).

This rebellion was interpreted as the monetarist counter-revolution. In a later essay Johnson (1976: 582) revised his concept of revolution by distinguishing two perspectives:

“One view is that the intent and the result is to change the political and social leadership of the society ... The other view is that the intent and result is to rid the society of control by outmoded forms of organization and ways of doing and deciding things, in order to free the society to cope more realistically, rationally, and effectively with its problems”.

The first perspective continues to be called ‘revolution’, whereas the second is termed a ‘war of independence’, which Johnson attributes to Keynes. In this interpretation, the ‘war of independence’ culminating in the publication of the *General Theory* merely marks the final stage of an *evolutionary development* from a barter economy to a monetary exchange economy and, finally, to a monetary economy:

“From that point of view, the so-called Keynesian Revolution has to be viewed as the third stage of evolution of the theory of a monetary (and capitalist) economy ... a stage lasting for something like fifteen years after the publication of The General Theory, culminated in the rather unsatisfactory synthesis of acceptance of the 'full employment' policy objective and the Keynesian analytical model, together with the admission that 'underemployment equilibrium' was an abstract theoretical special case dependent on either a short-run approximation of wage rigidity or some very peculiar and not seriously empirically defensible behavioural assumptions” (Johnson 1976: 584 und 589).

The contribution of Victoria Chick (1978) takes up Joan Robinson’s attempt to explain why Keynes’s revolutionary claim was never fully realised. Whereas Robinson focused on the assimilative tendencies of the Hicks–Hansen ‘standard Keynesianism’, Chick addresses Clower’s ‘rationing Keynesianism’. Clower had emphasised that the rejection of Walras’s Law was crucial to the revolutionary claim of Keynesian theory and attempted to show that, at least in cases of ‘false-price trading’, Walras’s Law would be refuted. In this interpretation Keynes becomes not a ‘short-run theorist’ but rather a ‘disequilibrium theorist’.

Chick, however, neither employs philosophical reflections à la Kuhn to assess Clower’s rationing Keynesianism analytically nor tests the consistency of his claim that Walras’s Law can be rejected — at least under disequilibrium conditions. Instead, she criticises Clower’s traditional *concept of equilibrium* — as the equality of supply and demand — and proposes an alternative concept:

“Equilibrium is a point of rest; forces leading to change are either absent or countervailing” (Chick 1978: 17).

Since every rationing outcome under false-price trading may represent an equilibrium of this kind, and since economic agents lack information about market-clearing prices, an underemployment equilibrium becomes a conceivable outcome of the market process regardless of whether (notional or effective) supply and demand are equal. The demonstration that Walras’s Law does not hold under these conditions appears to Chick almost trivial:

“Is it any wonder that Walras’ Law does not hold!?” (Chick 1978: 5).

According to Chick (1978: 3, my italics), the revolutionary core of Keynesian theory, therefore,

“lies in the choice of analytical method. Keynes applied a rudimentary process analysis to an economy characterised by a definite sequence of decisions and events through time ... As a result of this *change of method*, it is no longer appropriate to treat ‘equilibrium’ as synonymous with the absence of excess demand and supply in every market”.

A similar argument is advanced by Johnson et al. (2004), who maintain that the revolutionary aspect of Keynes’s work — described entirely in Kuhnian terminology, though without explicitly citing Kuhn — lies in the use of a different *concept of equilibrium*, that is, a methodical rather than a substantive theoretical-epistemological innovation. According to their interpretation, John Maynard Keynes does not employ an allocative concept of equilibrium — i.e., one related to market clearing — but rather a stability-oriented concept, according to which aggregate (planned) supply and aggregate (planned) demand can be in equilibrium at any level of employment:

“As such, macroeconomic equilibrium is defined in terms of relationship between ex ante saving and ex ante investment, and macroeconomic equilibrium is achieved when the sum of ex ante leakages equal to the sum of ex ante injections into the income stream. Thus, Keynes created the possibility that ex ante savings, a leakage from the income stream, might not be converted into an equivalent amount of ex ante investment, an injection into the income stream. Hence, increases in ex ante savings (decreases in ex ante consumption) became a source of deficient aggregate demand, resulting in involuntary employment in equilibrium” (Johnson 2004: 227).

However, Keynes himself had already pointed out that this type of *temporary* equilibrium is entirely *compatible* with standard economics and does *not* constitute a contradiction of Say's Law:

“For ‘Supply creates its own Demand’ must mean that $f(N)$ (aggregate demand, A.H.) and $\phi(N)$ (aggregate supply, A.H.) are equal for *all* values of N , i.e. for all levels of output and employment. ... the aggregate demand price (or proceeds) always accommodates itself to the aggregate supply price; ... That is to say, effective demand, instead of having a unique equilibrium value, is an infinitive range of values all equally admissible; and the amount of employment is indeterminate” (Keynes 1936: 25f., italics in the original).

Ivo Maes and Erik Schokkaert (1986) also interpret the Keynesian Revolution — again without any reference to philosophy of science — as a methodical phenomenon. Yet their concept of revolution does not even involve an intense or sudden change; rather, it refers to the development toward greater *formalization* (mathematization, econometrisation) and *empirical testing*, which is commonly understood, rather, as the *professionalization* of the discipline.

As already mentioned, none of these contributions refers to the insights of Thomas S. Kuhn and the implications arising from the publication of *The Structure of Scientific Revolutions*. This is all the more surprising given that Kuhn had by then already been cited within economics. For example, A. W. Coats (1969) affirmatively answered — already by the late 1960s — the question that Kuhn himself had left open, namely whether economics could in fact be understood as a paradigmatic science. It was not until James R. Stanfield attempted in 1974 to interpret the Keynesian Revolution in Kuhnian terms.

Affirmation when a philosophy-of-science perspective is taken

Stanfield's focus lies in reconstructing the sequence of a paradigm shift — from the empirical anomaly of the Great Depression to the publication of *The General Theory of Employment, Interest and Money* as an alternative perspective — according to Kuhn's framework, while also demonstrating the revolutionary character of Keynes's theory. This revolutionary character would be marked by a *paradigm shift* involving the following general principles:

“the new paradigm is not simply a cumulative process of attaching a new layer to the old foundation. Rather, it is the construction of a new foundation involving new fundamental laws, generalizations, and behavioral functions, often new methods and applications, and a redefinition of the character and standards of the science. The new paradigm is *a change in world view* to the extent that the world itself is changed: The perception and cognition of data and even the data to be collected are redefined” (Stanfield 1974: 100; my italics).

Conceding an affirmative perspective on the Keynesian Revolution, Stanfield must therefore interpret it as *a change in world view*. But what does this mean in concrete terms? Does it imply a new ontological understanding of the capitalist economy whose laws of motion demonstrably invalidate Say's Law or Walras's Law, thereby establishing the incommensurability of the new paradigm with the old paradigm as a prerequisite for a genuine paradigm shift? For Stanfield (1974: 102f.; my italics), the *new world view* consists rather in a *new perspective* on capitalist economics — that is, a change in the *observer's standpoint* rather than *a change in the fundamental understanding* of the object under observation:

“Turning to the General Theory, one can first see that its revolutionary Kuhnian Scientific Revolutions content lay not solely in the advocacy of counter-cyclical fiscal policy, the use of aggregate analysis, the store of value function of money, the possibility of money illusions or gluts, nor the distinction between savers and investors, all of which had been mentioned before. Rather, Keynes combined these factors in a way that *altered the focus of the discipline*. ... After Keynes, the center of macroeconomics was to be the level of employment of resources, the primary role of aggregate demand in determining this level, and the role of government in underwriting the adequacy of aggregate demand”.

And for those who still find this explanation vague or cloudy, Stanfield (1974: 104; my italics) becomes even more explicit:

“The fundamental *change in world view* evoked by the Keynesian revolution is the *attention centered on* the level of employment, income, and output, not on *any particular explanation* of this level nor on any particular policy to achieve a desirable level”.

Contrary to his own intention, Stanfield thus claims that the Keynesian Revolution — if it is really to be understood merely as a shift in perspective — does not represent a scientific revolution in the philosophy-of-science sense, replacing an old paradigm with a new, incommensurable one. Rather, it would at most describe another *variation* within a still dominant paradigm. Accordingly, Stanfield does not even attempt to argue that Say's Law, Walras's Law, or other indispensable features of the old paradigm should be regarded as refuted.

Apart from Stanfield, only three *[sic!]* further works analyse the Keynesian Revolution through the lens of Kuhn and the philosophy of science: those by Mehta (1979), Blaug (1991) and

Tuncer (2008). Mehta's work might perhaps not merit further consideration were it not for the fact that it refers to a view occasionally expressed in connection with the Keynesian Revolution: namely that Keynes's theory may indeed be described as revolutionary, but that this *does not imply a radical break* with the previously dominant theory:

"Keynesian normal scientists have not aimed at fundamental theoretical novelties" (Mehta 1979: 157).

This statement is significant because Mehta — incorrectly — considers it compatible with Kuhn's concept of a scientific revolution, which precisely presupposes a paradigm shift and regards different paradigms as incommensurable. Mehta supports his claim by drawing a comparison with the Copernican Revolution and its founder Nicolaus Copernicus:

"In many other respects Copernicus was a traditional astronomer. Apart from denying that the earth is at rest he did not radically depart from Ptolemaic tradition. For example, he continued the use of epicycles, eccentrics and other Ptolemaic devices to account for the motions of the heavenly bodies. There is nothing incongruous in this. It appears to be incongruous only to a catastrophist who thinks that *revolutionary works must instantly overthrow everything* from the past" (Mehta 1979: 159; my emphasis).

In fact, a scientific revolution in Kuhn's sense does not imply a particularly rapid paradigm shift — contrary to what e.g. Harry G. Johnson (1971) assumed — nor the rejection of *every conventional* epistemological tool, but *it does require the incompatibility of the ontological foundations*. The Copernican Revolution did *not* consist in proposing variations in epicycles or orbital paths to explain the observed constellations within the geocentric *worldview*; rather, it consisted in the complete *transition* from a *geocentric* to a *heliocentric* system. All adherents of the new heliocentric model agreed precisely on this point. In fact, supporters of the geocentric model attempted for almost a century to *defend the geocentric worldview* by modifying assumptions about epicycles and orbital paths — thus making adjustments in the peripheral protective belt of assumptions. Ultimately, this effort was bound to fail, as the available technical instruments had become too precise to withstand empirical refutation²⁴. In this way, Mehta — perhaps unintentionally — was the first to address explicitly something that had previously remained entirely overlooked: the reference to the *ontological dimension of a paradigm*.

Mark Blaug's (1991) article is interesting because it offers a philosophy-of-science-informed analysis of the Keynesian Revolution, while explicitly drawing not on Thomas Kuhn but on Imre Lakatos instead. He considers Lakatos's approach of *scientific research programmes* to be better suited to explaining the development of economics than Kuhn's paradigmatic approach (Blaug 1975). Unlike Kuhn, Lakatos describes the development of a scientific discipline as a competition between different research programmes, which are evaluated according to their ability to answer both established and newly emerging questions, and are thus classified as either *degenerating* or *progressive*. *Degenerating research programmes* tend not to attract

²⁴ It must, however, be acknowledged that even in astronomy, cosmic ontology remains normative and cannot be definitively falsified empirically — there are still proponents of the geocentric worldview today (see e.g. Bouw 1992) and there has been a long — in my judgement unresolved — discussion whether the shift towards the Copernican system was rational — on grounds of verisimilitude — or rather aesthetic (see e.g. Millman 1976; Westman 1980; Curd 1982).

further support, whereas *progressive research programmes* quickly gain followers. However, the concept of a *scientific revolution* does not appear in Lakatos's framework, because transitions from one research programme to another do not necessarily have to be understood as structural breaks resting on incommensurable ontological foundations. According to Blaug (1975: 399), this makes Lakatos's approach better suited than Kuhn's for understanding the development of economics—a discipline that, in his view, has not experienced a succession of scientific revolutions.

And this general assessment is also applied to the Keynesian Revolution: Keynesian theory was, for a time and with considerable consensus, perceived as a *progressive research programme* (which then increasingly *degenerated* with the emergence of 'anomalies' such as the stagflation of the 1970s) without being anything of a rupture that could be defined as a scientific revolution:

"... Keynesian economics sought not so much to supplant orthodox economics as to complement it" (Blaug 1991: 188).

Elsewhere, he made clear that he saw Keynes's innovation rather in methodical than theoretical or ontological innovations:

"The really novel aspects of Keynes, however, are, first of all, the tendency to work with aggregates and indeed to reduce the entire economy to three interrelated markets for goods, bonds, and labor; secondly, to concentrate on the short period and to confine analysis of the long period, which had been the principal analytical focus of his predecessors, to asides about the likelihood of secular stagnation; and thirdly, to throw the entire weight of adjustments to changing economic conditions on output rather than prices" (Blaug 1975: 413).

Ismail Tuncer sees — apparently in the sense of a Kuhnian paradigm shift — a revolutionary transformation in the *General Theory* relative to standard economics, triggered by the circumstances of the Great Depression and manifested in a theory implicitly rejecting Say's Law:

"... in the *General Theory* and the *Treatise*, Keynes intended to deal with two things. The first one was to show that the existing orthodoxy was inappropriate for dealing with any economic situation other than the special case of the full employment of resources. Second, he aimed to provide an alternative theory of the working of a monetary production economy which would be general, in the sense that it would demonstrate that the system could be in equilibrium at any level of employment... And by doing this Keynes provided a new 'gestalt' to the economic sphere" (Tuncer 2008: 169).

However, Tuncer does not clarify how Keynes justifies the rejection of Say's Law, and two aspects raise doubts as to whether the Kuhnian lens has enabled him to see sharply enough. First, he applies the Keynesian revolution equally to the theoretical considerations of the *Treatise on Money* and the *General Theory*. Yet for Keynes himself it was clear that the decisive break — if he indeed intended a paradigm shift — lay *between* his *Treatise on Money* and the *General Theory* (Keynes 1936: xxi). Second, Tuncer employs the concept of *Gestalt*, apparently as a synonym for Kuhn's concept of a paradigm, but seems to interpret a *Gestalt shift* again merely as a *change in viewpoint*, not as a *transition* between *incommensurable* analytical frameworks.

The most detailed analytical approach to the Keynesian Revolution is provided by Paul Davidson (1980; 1982), one of the founders of American Post Keynesianism. As the basis for his thesis that both the standard Keynesian (John Hicks, Alvin Hansen, Paul Samuelson) and the rationing Keynesian (Robert Clower, Axel Leijonhufvud, Edmond Malinvaud) interpretations have misunderstood the revolutionary core of the *General Theory*, he examines the epistemological core of orthodoxy and contrasts it with a Keynesian alternative that later became known as monetary or fundamentalist Keynesianism (Alan Coddington 1976).

Without explicitly referring to the philosophy-of-science literature, Davidson employs concepts such as paradigms and paradigm shifts. He points not only to the rejection of Say's Law and Walras's Law as a *fundamental condition* of a genuine scientific revolution, but also identifies the core axioms of the neoclassical market paradigm that guarantee the dynamic stability of general equilibrium and must, therefore, likewise be rejected: the gross substitution axiom, the axiom of reals, and the ergodic axiom.

These axioms, which constitute the negative heuristic of the market paradigm (see Chap. 3), must be rejected because in the alternative paradigm described by Keynes — and Davidson — as a monetary production economy, a good in which forward contracts are denominated — money — acquires a significance that goes far beyond its role as a medium of exchange or numéraire. It possesses distinctive properties: low elasticity of substitution and low elasticity of production (see Keynes 1936: Chap. 17). These properties characterize a social interaction space defined by fundamental uncertainty. Thus, it is the combination of time-consuming, unidirectional production processes, uncertainty, and the use of money that produces an economic system fundamentally different from an (equally monetary and intertemporal) exchange economy. Consequently, such a system can only be captured by a theoretical approach that must be incommensurable with neoclassical equilibrium economics:

“The Keynesian revolution in economic theory was therefore truly a revolt since it aimed at rejecting basic mainstream tenets and substituting postulates which provide a logical foundation for a non-Say's Law model more closely related to the real world in which we happen to live. ... In a world with important non-ergodic circumstances—our economic world— liquidity matters, money is never neutral, and neither Say's Law nor Walras's Law is relevant... In such a world, Keynes's revolutionary logical analysis is relevant” (Davidson 1984: 561f., 572).

Davidson — drawing again directly on Keynes — compares traditional economics to *Euclidean geometry*, which must be replaced by a *non-Euclidean geometry* (Keynesian economics):

“The classical theorists resemble Euclidean geometers in a non-Euclidean world...” (Keynes 1936: 16).

However, this analogy is problematic because, in the philosophy of science, the relationship between Euclidean and non-Euclidean geometry is generally *not* considered an example of a *paradigm shift* but rather a *paradigm extension*, since non-Euclidean geometry *includes* Euclidean geometry as a special case (see e.g. Torretti 1978). In this interpretation, the *General*

Theory would not represent an *incommensurable alternative* but merely a *commensurable generalization*²⁵.

Further assessments of the Keynesian Revolution

Finally, a number of studies—including those by Robert Eisner (1975), E. Roy Weintraub (1975), Donald Moggridge (1986), James Tobin (1988), Peter Rosner (1996), Hansjürgen Klausinger (1999), Roger Backhouse (2002), Nahid Aslanbeigui and Guy Oakes (2002), Tomo Suzuki (2003), and Luca Fiorito and Matías Vernengo (2009) — do not primarily examine whether and in what sense the *General Theory* should be understood as a revolution. Rather, they retrospectively ask what the distinctive characteristics of Keynesian economics are, why an apparently crucial analytical category such as uncertainty largely disappeared from dominant versions of Keynesian economics, what impact Keynes's work had, what roles Harvard played in its American reception and the 'Cambridge Circus' in stirring the revolution, and how various related developments shaped macroeconomics and economic policy.

Moggridge is, still, interesting insofar as he insinuates that not only do most references to the *General Theory* adopt a critical or revolution-denying stance (which is incorrect, see Tab. 1), but that Keynes himself may have intended his revolutionary claim primarily as rhetoric (Moggridge 1986: 361)²⁶. Rosner, for his part, identifies scientific progress even outside revolutionary innovations — without mentioning the Kuhnian distinction between scientific work (and progress) *within* a paradigm ("normal science") and scientific work (and progress) aimed at a *paradigm shift* ("revolution").

6. Discussion

As might hardly be expected otherwise, assessments of the impact and significance of Keynes's *General Theory of Employment, Interest and Money* are far from uniform. The studies examined can be classified, on the one hand, according to whether they attempt an *analytical* treatment of the phenomenon of the Keynesian Revolution based on philosophy- of-science principles or merely understand it *intuitively* as something 'new' or 'different'. On the other hand, they can, of course, also be differentiated according to whether the reference is *affirmative* or *aversive*. Furthermore, this classification can be additionally subdivided into ontological, epistemological and methodological substantiations from which the affirmative reference is derived (see Table 1).

A look at the articles in academic journals whose titles explicitly address the Keynesian Revolution initially confirms the frequently expressed view that the affirmative attribution of a revolutionary influence of the *General Theory* on economics increased with the *passage of*

²⁵ And this is, of course, an interpretation that has frequently been proposed. Indeed, there are numerous passages in the *General Theory* (e.g., Keynes 1936: xxiii, 378f.) that suggest Keynes actually believed he had formulated a 'general' rather than an 'alternative' theory. On the other hand, there are at least as many passages (e.g., Keynes 1936: xxiv, xxix) that imply he intended to leave standard economics behind in a radical — indeed revolutionary — manner.

²⁶ This view is occasionally put forward, according to which it was not John Maynard Keynes's intention to revolutionize economic theory itself, but rather the way the world thinks about economic problems (see e.g. Dobb 1973: 214). This interpretation—or perhaps semantic sleight of hand—is explicitly not adopted here.

time since its publication (and was articulated in particular by the *younger generation* of economists; cf. Samuelson 1946; Moggridge 1995): Up until 1962, the year *The Structure of Scientific Revolutions* was published, 3 out of 5 articles dealing with the Keynesian Revolution rejected the revolutionary attribution²⁷; thereafter, 12 out of 17 endorsed it (see Table 1).

The fact that economists, even decades after the ‘Kuhnian revolution’ in the understanding of scientific progress, appear to have taken little notice of these developments in the philosophy of science — or at least have made only insufficient use of them when addressing the evaluation of Keynes’s theory with only 4 out of 16 articles drawing explicitly on Kuhn (see Table 1) — confirms the assessment that most professional economists display a certain *lack of familiarity* with philosophy-of-science issues. It also suggests that they have tended to engage with the development of their discipline in a rather anecdotal manner or, from a professional perspective, at best in terms of the history of economic doctrines. Even the few cases in which economists have explicitly drawn upon philosophy-of-science insights raise concerns that a deeper, propaedeutic understanding of the preconditions and the significance of genuine scientific revolutions is lacking when none out of 3 articles point to the ontological dimension of a new paradigm in the making (see Table 1). However, where a more analytical stance has been taken — whether with explicit reference to Kuhn or merely implicitly — the rejection of the validity of Say's Theorem or Walras's Law played a central role.

This rejection was either merely invoked as a defining criterion (Mehta, Rosner, Tuncer) or attempts were made to reconstruct it in the form of IS–LM, rationing, or Post Keynesianism. While the Post Keynesian Davidson *transforms* the epistemological foundations of the market paradigm more fundamentally by replacing its core axioms, the reconstruction attempts of the standard Keynesian Klein and the rationing Keynesian Clower remain explicitly *within* the framework of the market paradigm. What may be interpreted as an attempt at assimilation — and which likely formed the basis for the wide acceptance and reception of these reconstructions as opposed to the marginalization of Post Keynesianism — was accordingly met by Post Keynesians with the accusation of *inconsistency* and held responsible for a ‘*revolution that never was*’. From a philosophy-of-science perspective, this verdict must be accepted, since peripheral epistemological modifications without a transformation of the core axioms — something that must ultimately also manifest itself in the ontological foundations of the paradigm — produce at most *axiomatic variations*, but not a *paradigm shift*.

Those studies that argue more intuitively (e.g. Harry G. Johnson, Victoria Chick, Donald Moggridge, Ivo Maes and Erik Schokkaert) locate the revolutionary aspect of Keynesian economics in innovations that altered the discipline in a lasting—and comparatively rapid—way. These are usually methodological changes (for example, a macroeconomic rather than microeconomic perspective, a new concept of equilibrium, or increasing formalization), the significance of which is often measured by their reflection in textbooks, making them appear different from those published before the revolution (Peter Rosner).

²⁷ And this does not even include all those previously mentioned numerous critical receptions that do not carry the term ‘Keynesian Revolution’ in their title for that very reason.

Table 1: Classification of assessments (N = 31)

	Assessment					
Methodology			Revolution (N = 14)			No Revolution (N = 8)
			Ontological (N = 2)	Epistemological (N = 5)	Methodological (N = 7)	
	Kuhnian Analysis (N = 4)		-	Mehta (1979)	Stanfield (1974); Tuncer (2008)	Blaug (1991) —
	Intuitive or without Kuhnian references (N = 16)	After 1962 (N = 13)	Davidson (1980); Davidson (1984)	Klein (1964); Clower (1965); Plassard (2018)	Johnson (1971); Johnson et al. (2004); Chick (1978); Maes and Schokkaert (1986)	Robinson (1974)*; Johnson (1976); Moggridge (1986); Rosner (1996)
		Before 1962 (N = 5)	-	Klein (1944)	Browne (1946)	Knight (1937); Gambs (1947); Dillard (1947)

Notes: * Revolution that never was position; Eisner (1975); Weintraub (1975); Tobin (1988); Klausinger (1999); Backhouse (2002); Aslanbeigui and Oaks (2002); De Vroey and Malgrange (2012); Fiorito and Vernengo (2009); Toporowski (2017) (N = 9) not classified because they provide no reasoned evaluation; Suzuki (2003) not classified because he refers to a policy revolution, not to a theoretical revolution

If one compares affirmative with critical assessments, not only does affirmation clearly predominate quantitatively (14 pros : 8 cons) — the Keynesian Revolution is frequently even used as a synonym for Keynesian economics (e.g. Robert Eisner, James Tobin, Roger Backhouse, Ivo Maes and Erik Schokkaert, Michel De Vroey and Pierre Malgrange, Luca Fiorito and Matías Vernengo) — but rejection and affirmation are sometimes justified in similar ways due to a lack of analytical precision. For example, G. W. G. Browne interprets the asserted methodological shift in perspective as a revolution, whereas Dudley Dillard, Harry G. Johnson, and Peter Rosner regard it rather as an evolutionary development within the prevailing paradigm.

Above all, however, even a shared affirmative verdict regarding the revolutionary content of the *General Theory* does not imply agreement concerning the evaluation of the Keynesian Revolution itself. This relates to the distinction between *Keynesian economics* and *Keynes's own economics*. While Post Keynesians see the *General Theory of Employment, Interest and Money* as a fundamental critique of neoclassical economics and therefore regard the standard and rationing Keynesian reconstructions—still embedded in the market paradigm—as inadequate and, in philosophy-of-science terms, as merely *intra-paradigmatic variations* rather than genuinely revolutionary, those very approaches consider their variations

sufficiently new and distinctive to justify speaking—at least in a colloquial sense—of a revolution.

7. Conclusion – the Revolution that was – or wasn't

The evaluation of *The General Theory of Employment, Interest and Money* as spurring a 'Keynesian Revolution' is by no means uniform in the economic literature. While early assessments often regarded the theory of John Maynard Keynes at best as a development *within* the existing neoclassical market or real-exchange paradigm, the view later gained ground that it represented a revolutionary break in the history of economic thought.

A systematic meta-synthesis of all journal articles that, by reference in their titles, indicate that they deal with the Keynesian Revolution shows, however, that this attribution is usually justified more *intuitively* than on the basis of *philosophy-of-science reasoning*.

Even after the publication of Kuhn's analysis of scientific revolutions, a methodologically grounded evaluation of the Keynesian Revolution remained rare. Where a stricter analysis is undertaken, the assessment depends decisively on whether Keynes's theory is interpreted as a *fundamental transformation* of the *epistemological* and *ontological* dimensions of neoclassical economics or merely as a *modification* of it. Accordingly, the spectrum of interpretations ranges from viewing it as a genuine paradigm shift to considering it merely an intra-paradigmatic development.

Keynes's premonition that his academic fellows might be unable to disentangle themselves sufficiently from the embrace of the prevailing neoclassical theory — and would therefore judge the *General Theory* as containing either nothing new or even being entirely wrong — has only been partially borne out. For the vast majority of economists, the *General Theory* triggered a scientific revolution which, in the form of the standard and rationing Keynesian reconstructions, must in philosophy-of-science terms actually be regarded merely as an intra-paradigmatic variation. A minority of economists either deny the *General Theory* any revolutionary content whatsoever or argue that although the *General Theory* was revolutionary in character, its prevalent reconstructions domesticated it within standard economics and thus deprived it of its revolutionary impetus — '*the revolution that never was*'.

More conclusively, it may be stated that the consistent *rejection of Say's Theorem or Walras's Law* is accepted as a *necessary* and *sufficient* condition for a revolutionary transformation of standard economics. What remains contested, however, is whether such a rejection can occur within a market- or exchange-theoretical ontology or need a clearly defined, yet different ontology.

The *Copernican Revolution* is often cited as a prime example of a scientific revolution. In this case, the core of the revolution is clear: it consisted in a shift in the ontological understanding of the object of inquiry — or, in short, in the pre-analytical vision — from a *geocentric* to a *heliocentric* model. At the same time, it has been shown how the Ptolemaic proponents of the geocentric system *resisted* this shift for a long time: by adjusting the protective belt of auxiliary assumptions in order to reject the Copernican model outright, or by seeking compromise and amalgamation. The model of Tycho Brahe stands as a representative example of the latter:

while geocentrism — the essential constituent of the Ptolemaic system — was preserved, elements of the Copernican system were nevertheless incorporated by conceding that the other planets revolve around the sun rather than the earth. Ultimately, it was *Johannes Kepler* and *Galileo Galilei* who, through their theoretical- epistemological and empirical contributions, brought about the *final breakthrough* of the Copernican Revolution. In this sense, proponents of standard and rationing Keynesian reconstructions may be understood as *‘Tychonists’*, seeking — intentionally or not — to *preserve the exchange paradigm* at its core while simultaneously *incorporating Keynesian elements* — through the linkage of the real (IS) and monetary (LM) spheres or through the dual-decision hypothesis (effective demand). The question that remains still open is: Will the discipline of economics witness its own Keplers and Galileis who will bring the Keynesian Revolution to completion?

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Appendix

Table A.1: The Keynesian Revolution in the mirror of economic literature up to 1962

Author	Focus	Feature	Assessment	Comment
Knight 1937	Review of Keynes GT	Agrees with Keynes that, depending on interpretation of his difficult writings, either nothing new is said or he is completely wrong	No revolution	Keynes' GT can at most be understood as an imbalance or business cycle theory
Klein 1944 / Klein 1947	Multiplier process; policy change	IS equilibrium driven by income rather than interest mechanism; rejection of Say's Law; demand increase via 'deficit spending' and wage increases/redistribution	Revolution, because aggregate output level is not assumed to correspond to full factor utilization, but receives its own theoretical basis (aggregate demand)	Epistemological variation that does not justify a revolution; rejection of Say's Law unconvincing; describes the ISLM model as 'Keynesian system,' which we now know is not a 'revolutionary' break
Browne 1946	Review of GT	Lack of self-regulation, implying stabilization policy as a revolutionary new policy orientation	Revolution due to macroeconomic perspective and new policy regime	Methodological variation producing a new theory of interest but no new, incommensurable paradigm
Gambs 1947	Competitive equilibrium	Revolution in a Veblenian sense, understood as rejection of deductive method and rigid assumptions	No revolution, as methodologically follows same paths as neo-classical theory	In the sense of American Institutionalism, 'revolution' is seen as rejection of methodological basis
Dillard 1948	Review of books celebrating the 'New Economics' of Keynes	Insufficient demand as a starting point for economic policy intervention	No revolution, but a useful and fruitful extension of neoclassical economics	

Table 2: The Keynesian Revolution in the mirror of the economic literature after 1962

Author	Focus	Feature	Assessment	Comment
Klein 1964	Keynes's economic system	Underemployment equilibrium (implicit rejection of Say's theorem without mentioning Say's theorem)	Revolution	Extension of the IS-LM model without presenting new arguments for rejecting Say's theorem. No reference to Kuhn
Clower 1965	Dual decision hypothesis	Asks what must distinguish Keynesian analysis from classical analysis in order to genuinely speak of a revolution	Revolution, because Walras's Law does not possess universal validity	Investigates the logical foundations of the Keynesian revolution, which must lie in a clear distinction from the existing framework

Johnson 1971	Revolutions in economics	"unexpected speed of change"	Revolution that became a vulnerable orthodoxy	Completely unsystematic and analysis-free use of the concept of revolution, which labels every change as a revolution
Robinson 1974	Evaluation of the reception of the <i>General Theory</i>	Acceptance through sublimation and vulgarization. The new theory is assimilated into the old orthodoxy and thereby absorbed	A revolution that never was	No reference to Kuhn. Assimilation made possible through simplifications and adherence to orthodox building blocks or assumptions
Stanfield 1974	Keynesian revolution in a Kuhnian perspective	Change in worldview from allocation to income and employment provision	Revolution	Kuhnian perspective based on the idea that a change in worldview—from allocation to provision or resource management—occurred. However, the issue of incommensurability (refutation of Say's law) is not addressed
Eisner 1975	Keynesian economics	Liquidity and investment traps as a particular explanation of "unemployment equilibrium"	No judgement	"Revolution" mentioned exactly once in the text—in the sense of the Keynesian macro model with stabilization policy implications. No reference to Kuhn and no feature identified that would indicate a revolutionary innovation
Weintraub 1975	Uncertainty in Keynesian economics	Uncertainty as an essential phenomenon in a world where important economic decisions—investments—create a link between present and future	No judgement	The "Keynesian revolution" is not examined for its revolutionary novelty; instead the question is raised why an important Keynesian category—uncertainty—was largely lost in later interpretations of the Keynesian system
Johnson 1976	Sociological classification of the Keynesian revolution	"Keynesian revolution" as a long evolutionary process	Evolution rather than revolution	Instead of referring to Kuhn's theory of scientific revolutions, the concept of revolution is interpreted politically
Chick 1978	Assimilation of Keynes's economics into orthodoxy	Keynes's <i>General Theory</i> contains a methodology standing in revolutionary contrast to orthodoxy—thereby altering the methodological concept of equilibrium	Revolution that never was	She struggles to distinguish her interpretation of Keynes from Clower's: in Clower, Walras's Law does not hold, but only in disequilibrium. She attempts to show that it does not hold in equilibrium—not by offering a different epistemological structure but merely by redefining equilibrium
Mehta 1979	Evaluation of the Keynesian revolution in a Kuhnian perspective	"abortive lacking" as a refutation of Say's law because savings may not be channelled into consumption	Revolution	"Abortive lacking" theory surely non-Keynesian and surely non-revolutionary if temporary hoarding is meant. Interesting mainly for the observation that revolutions may not be instantaneous paradigm shifts but may require long periods and may involve incomplete breaks with the old paradigm
Davidson 1980	Axioms to be refuted	Axiom of gross substitution—a core axiom of social exchange—	Revolution	Although Kuhn is not mentioned, the axiomatic core of different paradigms is explicitly addressed

		untenable in a monetary economy where money has zero elasticity of substitution		
Davidson 1984	Axioms to be refuted	Axioms of gross substitution, reals, and ergodicity must be rejected	Revolution	No reference to Kuhn, but the epistemological distinction from mainstream exchange economics is clearly articulated
Moggridge 1986	Re-evaluation of Keynesian economics		No revolution	Appears to argue that Keynes himself did not take his revolutionary rhetoric seriously
Maes and Schokkaert 1986	Methodology	Mathematization, econometrization, and positivization of economics	Revolution	Revolution interpreted as a gradual strengthening of already existing trends
Tobin 1988	Keynesianism in the US and at Harvard		No judgement	Essay on Harvard's influence in bringing Keynesianism to America. No discussion of the Keynesian revolution
Blaug 1991	Success of the Keynesian research programme	Defence of 'Keynesian Revolution' as the success of a temporarily 'progressive' research programme	No revolution	Casts the Keynesian revolution in Lakatosian instead of Kuhnian terms
Rosner 1996	Principle of effective demand	Rejection of Say's theorem	No revolution but still something "new"	Keynesian macroeconomics as "scientific progress"—not a revolution—because economics after the <i>General Theory</i> "knows more" (about the business cycle) than before
Klausinger 1999	German anticipations of Keynesian economics		No evaluation	"Keynesian revolution" and "Keynesian economics" used synonymously
Backhouse 2002	Patinkin's involvement in the Keynesian revolution	Patinkin as historian and theorist of the Keynesian revolution	No judgement	Article not about the content of the Keynesian revolution but about Patinkin's role in the formation of Keynesian economics
Aslanbeigui/Oakes (2002)	The formation of the 'revolutionary consensus'	The influence of the 'Cambridge Circus' on Keynes's theorizing and strategy	No judgement	Keynesian revolution as the product of a strategic fight for dominance
Suzuki 2003	National accounting system	Link between the revolution in national accounting and the macroeconomic revolution	Revolution	Revolution mainly in policy rather than theory; progress in national accounting as its foundation
Johnson et al. 2004	Concept of equilibrium	Equilibrium with (involuntary) unemployment	Revolution	Although Kuhn is not cited, the argument proceeds in Kuhnian categories—paradigm shift as the defining feature of a scientific revolution
Tuncer 2008	Applying Kuhn's theory to the Keynesian revolution	Implicit rejection of Say's theorem	Revolution	Sees a Gestalt shift in Keynes's <i>General Theory</i> relative to orthodoxy, though the precise object of this shift remains unclear

De Vroey / Malgrange 2012	Klein's interpretation of the <i>General Theory</i>	Involuntary unemployment as an equilibrium solution	No evaluation	"Keynesian revolution" used synonymously with Keynesian economics
Fiorito Vernengo 2009	J. B. Clark's evaluation of standard Keynesianism	Clark's version of the multiplier and his approach to business cycles	No evaluation	"Keynesian revolution" used synonymously with (standard) Keynesian economics
Toporowski 2017	Finance	"Finance"—as long-term bonds or equity—distinguished from the standard interpretation as "savings"	No evaluation	The Keynesian revolution is not even mentioned in the analysis; appears to be used synonymously with Keynesian economics
Plassard 2018	Clower's disequilibrium interpretation of the Keynesian revolution	Rejection of Walras's Law	Revolution	History-of-economic-thought attempt to trace the development of Clower's disequilibrium economics